

SCIENCE.

FRIDAY, MAY 20, 1887.

COMMENT AND CRITICISM.

WE MEET ALMOST DAILY with evidence of the increasing interest in folk-lore. There is, however, in some quarters a lack of specific knowledge as to the exact aims and methods of folk-lore studies, which must be removed before that general co-operation can be secured on which the success of these investigations is so largely dependent. Mr. Gomme, director of the English folk-lore society, is about to publish a book in order to present in simple and accessible form the needed information. This book will both supply collectors with suggestions as to what is required of them, and also form a scientific guide to the work of classification and comparison. Mr. Gomme points out the conditions of human life which would naturally give rise to religious beliefs, customs, and traditions, and then shows how the existence of such a thing as folk-lore is recognized when it is observed that there either exists or has existed, among the least cultured of the inhabitants of all the countries of modern Europe, a vast body of curious beliefs, customs, and narratives which are by tradition handed from generation to generation. These are essentially the property of the least-advanced portion of the community. They are neither supported nor recognized by the prevailing religion, by the established law, nor by the recorded history of the various countries. To this body of customs and beliefs there is constant addition made, arising from the explanation of newly observed phenomena by the uncultured portion of the community. The writer differentiates carefully savage custom and folk-lore, and says that the study of the former is necessary for the explanation of the latter. In not a few cases folk-lore is almost our only means of approaching the prehistoric period in the life of nations. Mr. Gomme offers as a definition of the science of folk-lore the following: it is "the comparison and identification of the survivals of archaic beliefs, customs, and traditions in modern ages." In this connection, it may be well to call attention to the letter, on another page, pleading for an American dialect society.

No. 224 — 1887.

THE AMERICAN ORIENTAL ASSOCIATION.

THE American oriental association met in annual session in the Athenaeum building, Boston, on Wednesday morning, May 11. In the absence of the president, Prof. W. D. Whitney, who, though considerably improved, has not yet, we are sorry to say, recovered his usual health, Vice-President Dr. A. Peabody presided. Considering the season fixed for the meeting, which is a particularly unfortunate one for those heavily engaged in university-work, the attendance was fair. Professor Lanman, in his report as secretary of the association, referred to the loss the society had incurred in the death of four of its members, — Professor Stenzler of Breslau (Germany); Dr. Alexander Wylie of London; Mr. H. C. Kingsley, treasurer of Yale college; and Prof. Charles Short of Columbia college. After speaking briefly of the services rendered by these gentlemen to the cause of learning, further remarks eulogizing the memory of the last named were made by Professors Thayer of Harvard, and Hall of the Metropolitan museum, New York.

The number of papers presented at the meeting was unusually large, a most welcome indication of the growth of oriental scholarship in this country. The reading of them, some merely in extract, consumed the greater part of the session, which lasted till late in the afternoon, with a short intermission at noon. Perhaps the most interesting of all was the first, by Dr. W. Hayes Ward, editor of the *Independent*, who offered a new and most happy interpretation of a scene depicted on a number of Babylonian seals which had hitherto baffled the ingenuity of scholars. On these seals we find a mythical figure in the act of ascending or resting his hands on what the late George Smith, the eminent Assyriologist, had taken to be a tower, but which Dr. Ward showed was a mountain. Behind the figure there is a portal out of which the personage ascending the mountain, or resting his hands upon the mountain-peaks, had evidently come. This scene, Dr. Ward proved, by a chain of arguments which left no doubt as to the correctness of his interpretation, is a symbolical representation of the rising of the sun, who at daybreak proceeds from the 'gate' behind which he was shut in during the night, and now climbs to the mountain-heights in order to illumine the world.

In the course of a discussion on this very suggestive paper, participated in by Professors Lyon

of Harvard college, and Jastrow of the University of Pennsylvania, further proofs were offered confirming, from certain references to the sunrise in the cuneiform texts, the mythological notions attributed by Dr. Ward to the Assyrians from a study of the seals in question.

Prof. Isaac Hall followed with an account of an important Syriac manuscript in the Union theological seminary of New York. Dr. Hall gave specimens of the manuscript, which will probably be published in the journal of the society.

Rev. Mr. Winslow had an interesting communication to make on the completion of an edition of the 'Book of the dead.' The publication of this, the most famous literary production of the ancient Egyptians, was undertaken at the instigation of the International congress of orientologists, under the superintendence of Prof. Eduard Naville of Geneva. An idea of the labor involved in this task may be gathered from the fact that the distinguished Swiss *savant* has been engaged in the preparation of it during the past twelve years. More than thirty papyri copies of the work were employed by him, besides the inscriptions on the walls at Thebes. As a result, two large folio volumes and one in quarto lie before us, containing the text, an elaborate introduction, and many thousands of variant readings. The old Egyptians carried the notion that this life was but a preparation for the next, to much further excess even than the Christians of the middle ages. Their entire philosophy and religion hinged around the one point of a future life; and hence it happens that their religious book *par excellence*, their Bible as we might say, was a 'Book of the dead,' or rather a 'Book for the dead,' containing an elaborate ceremonial and important instructions, all bearing directly upon death and the future world. The honors and distinctions which have been fairly showered upon Professor Naville since the completion of his enormous task, by the crowned heads of Europe and by learned societies, give proof of the great importance attached to this publication, which may indeed be said to mark an epoch in the history of Egyptology. At the suggestion of Mr. Winslow, the executive committee of the American oriental association adopted resolutions tendering its congratulations to Egyptologists and to Professor Naville, upon the appearance of the work.

Prof. D. G. Lyon announced a new publication in the department of Assyriology, which he believed to be as important to Assyriologists and Semitic scholars in general as the 'Book of the dead' was to Egyptologists. He referred to Prof. Friedrich Delitzsch's Assyrian dictionary, the first fascicule of which has just been issued.

The work had been announced already ten years ago, since which time scholars have been most anxiously waiting for it. Professor Lyon dwelt upon the enormous labor involved in it and on its great importance; but to judge from the first part, which only embraces a small portion of the first letter, the dictionary, when completed, will be of an enormous size, and it is doubtful whether it can be finished in less than a decade, to say the least, that is, if Professor Delitzsch continues it on the large scale mapped out in this fascicule. There is certainly no one better qualified for this prodigious undertaking — which, when completed, will be a monument to German scholarship — than Delitzsch, who is acknowledged to be without a superior as an Assyrian scholar; and his courage in not shrinking from the difficulties it involves is in itself worthy of our highest admiration. Assyriology is perhaps the youngest of the sciences. Scarcely three generations have passed since the first attempt was made to decipher a line of cuneiform writing; but so rapid is the progress which has been made, more especially during the past two decades, that such an undertaking as that of Delitzsch has become at least a possibility.

The afternoon session was opened with a paper on 'Ikonomatic writing in Assyrian,' from Prof. Morris Jastrow, jun., which treated on the transition from picture-writing to phonetic writing in the Assyrian cuneiform system in connection with a theory advanced recently by Dr. D. G. Brinton of the University of Pennsylvania. The latter assumed an intermediate stage between the method of expressing thought by pictures, and purely phonetical writing, to which he gave the name of 'ikonomatic writing,' and which consisted in using pictures or symbols for the purpose of indicating a word or words similar or identical in *sound* to the object represented by the picture. We find this method, which is the principle upon which the ordinary rebus rests, very widely employed in the Egyptian, Chinese, and in Mexican pictography. Thus, in the first named, *nefer* is the name of a lute, and represented by a picture of that instrument. But *nefer*, by a coincidence of sound (but not of stem), also signifies door, conscript soldier, and colt. Accordingly, by the adoption of the ikonomatic device, the picture of the lute is employed to recall any of these three words, though generally with some determinative sign as an aid to the reader to enable him to know which of the various *nefers* is meant. In the Mexican and Mayan systems, as Dr. Brinton shows, this method is carried to much further excess, a remote similarity of sound being sufficient to warrant the use of a picture or symbol in this way.

Professor Jastrow gave quite a number of instances to prove the existence of this phenomenon. He also suggested, that, with the aid of the ikonomatic method, certain phases of the *polyphonic* character of the cuneiform signs — according to which the sign *bn*, for example, could also be read *pn*; the sign *ag*, also *ak* and *ak*; and many more similarly — could be more satisfactorily accounted for than by assuming, as has hitherto been done, that such an interchange of consonants is purely arbitrary. Professor Jastrow, in conclusion, dwelt upon the bearings which all this had on the question as to the origin of the cuneiform writing.

After a discussion on this paper by Professors Lyon, Ward, and Toy, the last named read an interesting paper on the famous Lokman, the Aesop of the Arabs. After presenting the various theories that have been brought forward about this very mysterious personage, Professor Toy developed a new one, which may perhaps be best described as the legendary hypothesis with some historical basis.

Dr. Richard Gottheil of Columbia college followed with a learned disquisition on Baheira, and presented a careful copy of one of the Baheira legends in Syriac, which he had made from a Berlin manuscript. Besides this, Dr. Gottheil gave descriptions of two Syriac manuscripts now in New York. Papers were also presented by Professor Avery, Rev. Jos. Chandler, Dr. Jackson of Columbia college, and three by Professor Hopkins of Bryn Mawr college, which were only read in abstract by Professor Lanman. Dr. Selah Merrill, late U.S. consul to Jerusalem, made some remarks on the discovery of the second wall of Jerusalem and the bearings this discovery had upon determining the site of the holy sepulchre.

Before adjourning, the society elected the following officers for the ensuing year: president, Prof. W. D. Whitney; vice-presidents, Professors Peabody and Saulsbury and Dr. Ward; recording secretary, Prof. D. G. Lyon; corresponding secretary, Prof. C. R. Lanman; executive committee, Professors Thayer and Hopkins, President Gilman of Johns Hopkins, Dr. Hall, and Mr. Cothiel. A number of new members were elected, and it was decided to hold the next meeting in October, at either New Haven or Baltimore.

HEALTH MATTERS.

A RARE DISEASE. — The ship *Albana* recently arrived in New York from Manila. Of her crew of nineteen, seventeen became ill on the voyage, four of whom died. Five of the patients were

removed to the Long Island college hospital, where the diagnosis of beri-beri has been made. This disease has already been referred to in *Science* as a rare one, not occurring in this latitude except when imported from Japan, India, or other countries. It begins with fever, and pain in the loins and extremities: these are soon followed by paralysis of the lower then of the upper extremities, and sometimes extend to the muscles of the trunk, particularly those of respiration. The disease is in reality a multiple neuritis, involving especially the spinal nerves. The prognosis is usually considered grave, though the statistics of the Japanese navy would indicate that the mortality is not so great as is generally supposed. The cases that recover require considerable time, however, for the regeneration of the affected nerves. The medical director-general of the Japanese imperial navy has just published a report on this disease as it occurs in that branch of the service. In 1878 there were 1,485 cases, with 32 deaths; in 1879, 1,978 cases, with 57 deaths; during 1885 there were but 9 cases; and in 1886, only 3 cases, none of them being fatal. This remarkable result is attributed by the director to the improvement in the food furnished to the sailors. The few who contracted the disease in recent years had not had the benefit of the improved food.

SMALL-POX HOSPITALS. — One of the important subjects connected with sanitary administration is the location of small-pox hospitals in towns and cities. For the sake of the patients who are removed to these institutions, it is very essential that the distance which they are transported should be as little as possible; while, on the other hand, for the protection of the public, such hospitals should be situated so far from the inhabited portions of the town as not to be a factor in the spread of the disease. Mr. W. H. Power, a medical inspector of the English local government board, has given especial attention to this subject, and the evidence accumulated by him seems to indicate that a small-pox hospital in a town causes a greater incidence of small-pox in the vicinity. Within an area contained by a circle described three-quarters of a mile from the West Ham small-pox hospital, the death-rate from this disease was never less than twice, and sometimes even ten times, greater than the general rate; the number of cases showing a progressive decrease as the distance from the hospital increased. Mr. Power has studied the theories that the wind was a factor in this increase of cases near the hospital, and that the nurses and others were the channel of communication, but has found them inadequate to explain all the facts.

NOTES AND NEWS.

THE New England meteorological society has two special investigations on hand for the coming summer, in addition to its regular work of temperature and rainfall observation. The first special subject is thunder-storms in New England, now in its third year; the second is the sea-breeze on the eastern coast of Massachusetts, now undertaken for the first time. Volunteer observers are wanted in both investigations. Correspondence concerning thunder-storm observations should be addressed to O. N. Oswell, Cambridge, Mass.; concerning the sea-breeze, to L. G. Schultz, signal office, Boston, Mass. The considerable labor and expense that these studies involve is made possible to the society by assistance from the U. S. signal service, the Bache fund of the national academy, and the Harvard college observatory.

— Carl Gerold's Sohn, Vienna (New York, F. W. Christern), announces the publication of the 'Canon der Finsternisse,' by Th. Ritter von Oppolzer.

— D. O. Haynes & Co., Detroit, announce the publication of 'The cremation of the dead,' by Hugo Erichsen, M.D., honorary member of the Cremation society of Milan, Italy. It will contain an introduction by Sir T. Spencer Wells, and will treat of the question from an historical, sanitary, medico-legal, religious, aesthetic, and economical standpoint.

— The cornerstone of the Leland Stanford, jun., university, perhaps the most magnificently endowed institution of learning in the world, was laid at Palo Alto, Cal., May 13.

— In view of the renewed interest which the Central American states are now attracting, the article on Guatemala by W. T. Brigham, entitled 'An uncommercial republic,' which will appear in the June number of *Scribner's magazine*, will be especially timely and valuable.

— Mr. J. W. Graydon, late lieutenant U. S. navy, has recently been conducting some highly interesting experiments in Russia in the manipulation of explosives. He has been quite successful in mixing dynamite with ordinary gunpowder, obtaining greatly increased initial velocities without a corresponding increase in the pressure in the chamber of the gun. A valuable report upon the subject has been received at the navy department, which will soon be published.

— Mr. John Murray has in press a 'Dictionary of hymnology,' edited by the Rev. John Julian. The aim of the work is to trace the history of the Christian hymns of all ages, and especially of

those now used in English-speaking countries. Biographical notices will be given of the authors of the hymns, besides historical articles on liturgical music generally.

— Chapman & Hall will publish the English edition of M. de Lesseps' reminiscences, extending over a period of forty years, in October next, simultaneously with its appearance in Paris. The work will also appear in German, in the first instance as a serial in one of the Berlin papers.

— G. P. Putnam's Sons make the following additional announcements for the spring season: 'The curability of insanity and the individualized treatment of the insane,' by John S. Butler, M.D., late physician and superintendent of the Connecticut retreat for the insane; and, in the 'Questions of the day' series, 'The fishery question,' a summary of its history and an analysis of the issues involved, together with a full bibliography of authorities to be consulted, and a map of the fishing-grounds, by Charles Isham.

— The *Critic* states that a memorial volume of the late Prof. E. L. Youmans will be prepared by his brother and sister, W. J. and Eliza A. Youmans, and that it will contain a number of manuscripts and important letters, including his correspondence with Darwin, Spencer, Mill, Huxley, Tyndall, Lubbock, Agassiz, and other distinguished persons.

— An octavo volume of nearly four hundred pages, with facsimiles of the Charter of 1650 of Harvard college and of the first page of the College Records, with engraved views of the college in 1821, and containing a full report of the celebration of the two hundred and fiftieth anniversary of the founding of the college, is nearly ready for publication by the college.

— The Ladies' health protective association of New York has six hundred members on its roll. It proposes to devote its energies during the present year to tenement-house reform, and to improvement in methods of street-cleaning.

— A case of fatal cocaine-poisoning is reported in Germany, in which the patient, a girl aged eleven years, died within two minutes after receiving a hypodermic injection under the skin of the arm, consisting of from four to twelve drops (the exact quantity being unknown) of a four-per-cent solution of the drug.

— Ernest Ingersoll, in the June number of *The American magazine*, will describe the 'Last remnant of frontier,' — a portion of our country near the north-western boundary, which was an unsettled and almost unexplored wilderness until penetrated by the Northern Pacific railroad.

— Prof. J. P. Mahaffy has in press a book on social life in Greece. It includes a review of the life and thought in all the Hellenistic kingdoms from the time of Alexander to the Roman conquest.

— The study of mathematical astronomy seems to attract so few college students in this country, that we have examined with some interest a pamphlet just received, a thesis on 'Cometary perturbations' presented by Prof. W. Hoover for the degree of Ph.D., University of Wooster, O. About half of the pamphlet is devoted to a general discourse on celestial mechanics, and the rest to deducing certain well-known formulae for cometary perturbations. Considerable work has no doubt been done in the study of Watson's 'Astronomy,' but we should have been glad to see the actual application of the formulae to some of our recent comets, following the example of students of astronomy at the German universities.

LETTERS TO THE EDITOR.

**.*The attention of scientific men is called to the advantages of the correspondence columns of SCIENCE for placing promptly on record brief preliminary notices of their investigations. Twenty copies of the number containing his communication will be furnished free to any correspondent on request.*

The editor will be glad to publish any queries consonant with the character of the journal.

Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.

The Sonora earthquake.

In this portion of the world, since the afternoon of the 3d, have been occurring a series of seismic phenomena, such as, so far as records or tradition show, never have happened here. The telegraphic (newspaper) reports possess the usual characteristics, — gross exaggeration with utter inaccuracy of detail. I am securing data to aid in making a more complete record of the phenomena, and make this merely a preliminary note.

The earthquake struck this town at 3.06 p.m. local time, which, reduced to standard, gives 2.48. Probably the best description will be an account of my own sensations. I had just noted the time, when I heard a rumble such as made by heavy ore-teams in passing. This noise increased; and the building, a two-story adobe, began to shake gently, then more violently. By this time it seemed to me to be a severe whirlwind, such as frequently occurs here at this season of the year. The shaking and the noise increasing, I went to the front of the building, some fifty feet, and looked out. Then it began to dawn upon me that something of a serious nature was taking place, judging from the looks of the crowd on the streets. I then ran back to the place whence I had started, picked up a child, and made my way to the street. When the open air was reached, the noise was like a continuous roll of heavy firing, with occasional short peals like a sharp clap of thunder. This ceasing, I looked at my watch, and found that from the time I had noticed the first rumble until the end, had been about 1.75 minutes. Allowing

ten seconds for error, leaves 1.65. Of this time, the duration of the severe shaking could not have been over ten seconds; the moderately severe, about twenty; and the trembling, the balance of the time. Judging by the movement of some glasses and statuettes on my desk, the general direction of the shock was from south-west to north-east.

The amount of damage done, so far as I can learn, is trifling. No building of any stability has been damaged at all; neither has any one been injured or killed. Persons riding or driving were unaware that any thing was happening. In the Sulphur Spring valley, about twenty-five miles east of here, some fissures occurred in the bed of an old stream, and water spouted out to a small but varying height and in considerable quantity. These streams continued flowing for two or three days, but at present all save two are dry. These seem to be permanent, and are running a small amount of water at ordinary temperature. I have not seen them, and my account is derived from the owner. I may mention as an amusing fact, that, in less than an hour after they broke out, they were taken up and located under the water-laws of the United States.

Succeeding the shocks, mountain fires were noticed on many of the ranges. This gave rise to the reports of volcanic action, which may safely be set down as pure imagination. No phenomenon resembling eruptive disturbance, so far as I can now ascertain, has taken place in any part of the section disturbed.

At the time of the first and severe shock, owing to the vibration and the rolling of bowlders down the mountain-sides, large clouds of dust arose: this, with the noise, caused many who saw the phenomenon to think that the cause was eruptive. The fires, with only two exceptions that I now know of, were burning before the shock. Of these it is possible that they were not noticed prior to this, or, what in my opinion is not improbable, falling bowlders ignited the timber. This point I will try to clear up. The San Pedro River, a small stream nine miles west of here, is reported to have a slight increase of water. This is diminishing rapidly.

The railroad-track of the Atchison, Topeka, and Santa Fé road, at a point where it ran in an east and west direction, was bent $4\frac{1}{2}$ inches out of line, the convexity looking south. The bend was about three hundred feet in length. Succeeding the severe shock during the following forty-eight hours, marked and noticeable tremors occurred about every half-hour. These were of greater or less severity, but none approached the first. Had instruments been here to record the motion, no doubt they would have demonstrated a continual vibration.

The heaviest shock, since the first, occurred last night about 9.30. One ludicrous incident was that of an acquaintance, who, while driving along a mountain-road, noticed large-sized bowlders begin to start down the mountain towards him. He became much excited, took his rifle, and alighted with the intention of seeking vengeance on the perpetrators of the outrage. The falling of some immense bowlders weighing hundreds of tons just then, changed his mind: he will not now hunt the author. Men working at a depth of six hundred feet felt the vibrations severely. Some said they became sick, and all said that the bottom of the drifts or shaft seemed to rise. Men working at one hundred and fifty feet did not notice it so much. One crew of

men at that depth did not know of it at all until they came out of the mine. No damage was done to any of the mines. The deepest workings are seven hundred feet.

As an interesting coincidence, I will mention, that, while in the Salt River valley two weeks ago, I was informed by Mr. Frank Cushing the ethnologist, who is making extensive excavations in the old ruins abounding there, that one of the principal if not the main cause of the abandonment of so populous and fertile a valley was earthquakes. As there are no records of any occurring since that time, Mr. Cushing may take the blame of suggesting this. I am without trustworthy information concerning the extent of the disturbance; but, as near as I can judge, it is about twelve hundred miles long by six hundred in width. There were no magnetic disturbances whatever.

Since writing the above, additional information has come to hand that modifies my opinion somewhat as to the extent and character of this disturbance. From Señor Campi and Mr. L. A. Richards of Sonora, I am informed that the disturbance in their section of the country was profound. They are living in Sonora, Mexico, about two hundred miles south of here, in the Fronteras valley. The first shock was felt there about three o'clock on May 3. In Fronteras ten houses were thrown down, one child was killed, and one woman fatally injured. In Cumpas, still farther south, four houses were destroyed, no one hurt. Extending the entire length of the valley, over one hundred miles, are fissures varying in width from a few inches to ten feet, having a northerly and southerly direction. From this information it is safe to conclude that the centre or area of worst vibration lies to the south of this and in Mexico. It will take at least a month to secure requisite information to make a report. That country is sparsely settled, with no telegraphic communication or railroads; nothing but wagon-roads, and those very poor. These gentlemen confirm the report of mountain fires immediately succeeding the shock. They think that the entire valley has subsided a little.

Also at the San Bernardino ranch, ninety miles south-east of here, all the buildings on the place were thrown down. They were built of adobe, and were substantial. This place lies within a short distance of some extinct craters, and is in the centre of an ancient volcanic belt.

Later reports make the disturbance in Mexico, about the same region mentioned above, as very destructive. As some lack verification, I defer reporting them until further word is received.

G. E. GOODFELLOW.

Tombstone, A. T., May 7.

Defence of a civil academy.

The editorial columns of *Science* (May 13, 1887) are guilty of a manifest inconsistency upon the subject of state aid to the higher education. In your first column you condemn, in strong language, my idea of a civil academy at Washington, proposed in the circular of information, No. 1, 1887, bureau of education. In your fifth column you quote, with evident respect, Professor Jowett's views upon government aid to the university colleges of England. You even give publicity to this statement, without dissent: "No principle of political economy forbids

the application of public money to the education of those who cannot afford to help themselves. Such an expenditure is really one of the best affairs of business in which a nation can engage." You venture to add that there is some prospect of Dr. Jowett's plea being effective. While it is not to be expected, in the present transitional stage of political economy and in the present condition of American politics, that all men should agree upon the necessity of education and science for good government, it is at least fair to demand some degree of consistency in a scientific journal.

Furthermore, I beg to differ from your opinion that this country is dotted with colleges where any young man may obtain all needful political education. If there is one thing needful at the present time in our American civic life, it is instruction in the art of administration. Over against your statement, let me place that of Mr. Dorman B. Eaton, recently expressed before the graduate students of history and politics in the Johns Hopkins university. From his practical connection with the civil-service commission, he may be presumed to know what he was saying. He said there was not a single institution in the United States where a man could learn what reformers wish to know about scientific methods of administration. Mr. Eaton may have ignored one or two oases of political training in this country; but every fair-minded man must admit, upon reflection, that American colleges do not teach this subject. You say it is well enough to train men for the army and navy, but intimate that our prospective civil servants can acquire adequate training "from any village school, and will not ask the government for alms that they may the later live from the public purse." No, our public men sometimes try to carry the entire bag, and distribute public bounty, or 'spoils,' to all their friends and constituents. They even vote in state legislatures for free text-books in common schools, and allow publishers to corrupt school-committees. Who teaches 'the homely proverbs of Poor Richard' to our local politicians nowadays, and who ever heard of the A B C of finance in 'any village school'? Do the spoils system and the history of American legislation, municipal, state, and national, indicate that our public servants have been well grounded in common honesty and good political economy? Before pronouncing judgment upon my suggestion as *poor* economy, you might profitably compare the cost of scientific administration with the present American system. Materials for the comparison may be found in the civil lists of various European countries.

My plea was for a civil-service academy, recruited by congressional appointment from men pronounced fit by our state universities. It was for a civic West Point. It was for the political training of able and mature young men in a political environment, in the capital of the nation. It was a plea for opening the channel of communication between our universities and public life, between political science and political praxis. I proposed that the highest education in the country and the most expert talent now in the service of the government should both be made tributary to the training of picked young men for a term of two years, partly by lectures, and more especially by practical work in government bureaus, after the manner of the seminary connected with the Statistical bureau in Berlin, which is recruited by university graduates of the highest ability.

This is no visionary, unpractical scheme. It has been realized, in one form or another, by most European states. The idea is slowly evolving in connection with our own government departments. The state department has in training a body of consular clerks. The navy details men for special study in Greenwich, Paris, and Baltimore. The war department has also allowed men to study in Baltimore laboratories. Mr. Trenholm, the controller of the currency, says he is going to select the brightest young men he can find, and train them for bank-examiners. The idea is in the air at Washington, and it will sooner or later find a lodgement in every department and bureau. You will probably hear of it next week from Col. Carroll D. Wright, commissioner of the bureau of labor, in his address on the study of statistics in American colleges, before the American economic association, at its meeting in Cambridge, May 24, 1887. Statistical science, finance, forestry, agrarian economy, consular duties, and diplomacy have never yet been taught, to any considerable extent, in our American schools and colleges. You might as well expect a corps of military engineers to evolve from the state militia as to suppose that the higher arts of administration can be acquired by either school or college training. Administration is one of the highest branches of scientific politics, and it seems to me that *Science* ought to recognize the fact. As to the diplomatic service, a Boston gentleman, who has had much experience in this connection, writes, "I have had a good deal to do with some of our diplomatic servants in Europe, and have often been put to the blush for their incompetency to perform their duties. Why should we not have a diplomatic service like other nations, and why should we not have a national institution in which the students should be taught, among other things, diplomacy?"

HERBERT B. ADAMS.

Johns Hopkins univ., May 16.

The occurrence of similar inventions in areas widely apart.

The leading idea of Otis T. Mason's writings on ethnology is his attempt to classify human inventions and other ethnological phenomena in the light of biological specimens. "They may be divided into families, genera, and species. They may be studied in their several ontogenies (that is, we may watch the unfolding of each individual thing from its raw material to its finished production). They may be regarded as the products of specific evolution out of natural objects serving human wants and up to the most delicate machine performing the same function. They may be modified by their relationship, one to another, in sets, outfits, apparatus, just as the insect and flower are co-ordinately transformed. They observe the law of change under environment and geographical distribution." This method of research is founded on the hypothesis that a connection of some kind exists between ethnological phenomena of people widely apart. Professor Mason is of this opinion, and expresses it as follows: "Anthropologists assign similar inventions observed in different parts of the world to one of the following causes: 1. The migration of a certain race of people who made the invention. 2. The migration of ideas—that is, an invention may be made by a certain race or people and taught or loaned to peoples far

removed in time and place. 3. In human culture, as in nature elsewhere, like causes produce like effects. Under the same stress and resources the same inventions will arise." From this stand-point Professor Mason has arranged the ethnological collections of the national museum according to objects, not according to the tribes to whom they belong, in order to show the different species of throwing-sticks, basketry, bows, etc.

We cannot agree with the leading principles of Professor Mason's ethnological researches. In his enumeration of causes of similar inventions, one is omitted, which overthrows the whole system: unlike causes produce like effects. It is of very rare occurrence that the existence of like causes for similar inventions can be proved, as the elements affecting the human mind are so complicated; and their influence is so utterly unknown, that an attempt to find like causes must fail, or will be a vague hypothesis. On the contrary, the development of similar ethnological phenomena from unlike causes is far more probable, and due to the intricacy of the acting causes. As far as inventions are concerned, the disposition of men to act suitably is the only general cause; but this is so general, that it cannot be made the foundation of a system of inventions.

But from still another point of view we cannot consider Professor Mason's method a progress of ethnological researches. In regarding the ethnological phenomenon as a biological specimen, and trying to classify it, he introduces the rigid abstractions species, genus, and family into ethnology, the true meaning of which it took so long to understand. It is only since the development of the evolutionary theory that it became clear that the object of study is the individual, not abstractions from the individual under observation. We have to study each ethnological specimen individually in its history and in its medium, and this is the important meaning of the 'geographical province' which is so frequently emphasized by A. Bastian. By regarding a single implement outside of its surroundings, outside of other inventions of the people to whom it belongs, and outside of other phenomena affecting that people and its productions, we cannot understand its meaning. The only fact that a collection of implements used for the same purpose, or made of the same material, teaches, is, that man in different parts of the earth has made similar inventions, while, on the other hand, a collection representing the life of one tribe enables us to understand the single specimen far better. Our objection to Mason's idea is, that classification is not explanation.

His method, as far as applied to objects which have a close connection with each other, is very good. The collection of moon-shaped Eskimo knives or labrets from North-west America has given us great pleasure, and enables us to trace the distribution of those implements; but even they do not fully answer the purpose of ethnological collections. Besides these, we want a collection arranged according to tribes, in order to teach the peculiar style of each group. The art and characteristic style of a people can be understood only by studying its productions as a whole. In the collections of the national museum the marked character of the North-west American tribes is almost lost, because the objects are scattered in different parts of the building, and are exhibited among those from other tribes.

Another instance will show that the arrangement

of similar implements does not serve the purpose of ethnological collections. From a collection of string instruments, flutes, or drums of 'savage' tribes and the modern orchestra, we cannot derive any conclusion but that similar means have been applied by all peoples to make music. The character of their music, the only object worth studying, which determines the form of the instruments, cannot be understood from the single instrument, but requires a complete collection of the single tribe. Here, however, it can be seen that each ethnological collection affords only very fragmentary instruction; that its real use is only to illustrate descriptions of the tribes. For a study of native art and its development, they are indispensable. For this purpose, duplicates, of which the superficial visitor of ethnological museums frequently complains, are absolutely necessary. They are the only means of determining what is characteristic of a tribe, and what is merely incidental.

Mason's method takes a place in ethnology similar to the former 'comparing method' in geography. A mere comparison of forms cannot lead to useful results, though it may be a successful method of finding problems that will further the progress of science. The thorough study must refer to the history and development of the individual form, and hence proceed to more general phenomena.

DR. FRANZ BOAS.

New York, May 13.

Explosions in coal-mines.

In *Science* for May 6, is a review of the report of the Atkinsons on explosions in coal-mines. One or two statements therein seem to convey an erroneous impression; notably, "At the working faces the dust is not often a serious evil," and, under remedial measures, that "watering the roadways . . . is of little avail as a means of preventing explosions, since the upper dust in every instance is left undisturbed."

The first quotation is manifestly an error, as dust-explosions can generally be traced to the firing of the dust in the working faces by blown-out shots, especially when such shots react against a tamping of coal-slack. In the main body of the article the argument seems to be in favor of the dust in the gangways as the proximate cause of explosion, while it is but the ultimate cause. The dust formed by cutting or breaking down coal has very little to do with the formation of an explosive mixture, because it is not impalpable enough. As the article states, the gangway dust is ground to an impalpable powder, and carried away by the air; but such dust would do little harm, did the return currents through the working places not lose their velocity and deposit this impalpable dust on the walls nearest the face, from the fact that the ventilating currents must sweep the faces free from smoke and foul air. In the only known American accident due to dust (the Pocahontas explosion), there was little evidence of initial explosive force along the gangways, but in the headings of dusty rooms there occurred a series of explosions that made the fact evident that fine dust in headings is the cause of so many accidents.

The Prussian commission showed that a certain percentage of volatile matter was necessary for an explosive dust, and experiments made at this place show that the temperature at which coals give up their volatile ingredients vary; so that a blown-out shot, from a hole tamped with coal slack, projected

into a chamber whose walls are thickly powdered with fine dust, will have its flame prolonged by the dust of the tamping and the gases from that dust, and the case will be analogous to those stated by Professor Abel, where a small admixture of gas would render inert dusts explosive.

Finally, it has been found that watering dusty roads with brine at intervals of thirty days made the dusts less ready to rise in clouds, and stopped the formation of 'upper dust.' But the best way of all is to keep the roads clean by ballasting them at the outset with rock or shale free from carbon, and by picking off the coal shaken from cars. In the Pocahontas accident the explosive phenomena ceased as soon as the current left the region where the roads were ballasted with coal-slack, and the action in the parts ballasted with slate was a simple burning of the dust brought there by the current. In spite of an explosive coal, the parts of that mine last mentioned were comparatively free from dust, and the props nearly all standing, while in the former portion there was explosive action in nearly every working place, so that the tracks were torn to pieces and the props down. In mines of this nature, black powder should be avoided, and the coal should be wedged, or, if the coal must be shattered, the dynamite cartridge with water casing can be used with impunity.

EDWARD H. WILLIAMS, Jr.

Lehigh univ., May 13.

Water-filtration.

It may be of interest to notice in connection with your note on the results of Dr. Swarts's experiments on the relation of water-filtration to bacterial development, that Dr. J. H. M. Munro, in his experiments on the nitrification of well-waters, discovered and called attention to the fact that a well-water nitrified more rapidly after filtration through a Lipscombe's charcoal filter in common use, than did an unfiltered sample of the same water (*Journ. chem. soc.*, 1886, p. 666).

WILLIAM FREAR.

State college, Penn., May 16.

The fact that an increase of micro-organisms would take place in a filter constantly in use, had already been demonstrated by Percy Frankland, England, and by several German investigators.

The series of experiments conducted by myself differed from theirs merely in using the filters found upon the local market, and in imitating as near as possible their use in ordinary family water-supply; cleansing in the first use by allowing the supply to pass through the filter to wash away detritus, and not by absolute sterilization, as in usual laboratory experiments. The filters used were variously packed with bone or animal charcoal, quartz, the two combined in layers, felt, and unglazed porcelain.

Such a result as that spoken of in *Science* (ix. p. 457) is to be expected, when we consider the mechanical work we have to do in filtration; for no amount of chemical change is expected except in Clarke's process, which is precipitation, not filtration. If the filter is a successful strainer, the suspended matters within the water are held back upon the surface of the strainer and within the interstices of the filtering media, whether it be gauze, asbestos, iron shavings, felt, or porcelain. The great mistake seems to be in believing, that by use of a current of water, or by removing the media and scrubbing the surface with a brush, all the filtrate is removed, for-

getting the amount held within the interstices of the media.

In those filters in which the mechanism or media is reversed for cleansing, the organic matter upon which the microbes are feeding and multiplying, and which has become attached to the walls of the spaces of the filtering media, are not removed, any more than the greenish scum is removed from the stones in a rapidly flowing brook: on the contrary, so tenacious is this material, that it forms in strings and streamers pointing with the current.

As is well known, commencing at the set bowl in a dwelling-house, a deposit forms upon the sides of the waste-pipe, continues downward, adhering to the sides of the trap and continuing to the drain-pipe and sewer, till it reaches the point of delivery. This deposit is, of course, composed of the wastes which have been thrown into the bowl, and which is fully charged with organisms whose function is to destroy and assist in nature's retrograde metamorphosis. The strongest flushing of this pipe does not remove the slime from its sides: how, then, can a retarded pressure of water wash away the organic matter adhering to the sides of our meshes of felt and our granules of quartz and charcoal?

The number of microbes in a given sample of water serving to render it harmful, has not been actually determined, any more than a specimen can be condemned for the amount of albuminoid, ammonia, or chlorine alone which it contains; still a water containing over a thousand microbes or colonies to the cubic centimetre of water is the highest limit consistent with purity in drinking-water. A water which contains fifty bacteria to the cubic centimetre before filtration will increase to over a thousand in seven days' use, no matter how much care is taken to cleanse the filter short of absolute sterilization.

The point of danger, however, lies in the fact that the two diseases which are communicable by ingestion into the alimentary canal of the excrement from them (typhoid-fever and cholera) are the ones which are liable to find their way into drinking-water from contamination by sewage finding its way into river and well supplies.

I am at present conducting experiments to determine how rapidly the germs of typhoid may increase within filters in the presence of sterilized water and in presence of the bacteria of drinking-water.

GARDNER T. SWARTS.

An American dialect society.

Is it possible to establish such an institution? It is certainly time. Year after year the older districts of the United States and Canada are getting less and less distinguished by those peculiarities in their vernacular which to the student of history and philology are of the utmost interest. Public schools, many newspapers, cheap books, a taste for reading, a notion that 'old-time' ways and dialect are not 'elegant,' and, above all, the more constant communication between different parts of the country, are doing much to tone down the people of the United States to what, from the philologist's point of view, is one dead level. In time the mountaineers of Tennessee and the hill country of the Carolinas, the 'crackers' of Georgia, and the picturesquely talking folk of the Arkansas bottoms and the lower Mississippi, will have lost many of their present peculiarities of speech. Even the New-Englanders, I am

told (for I have not lived in America for more than twenty years), are fast abandoning many of those dialectic peculiarities which to a philologist are so suggestive. Even the Virginians, since they have gone into the great world, are no longer so readily 'berayed' by their speech. Now, therefore, is the time to collect vocabularies of these local dialects, with specimens gleaned from printed works illustrating the use of any particular word. Books, almanacs, election-addresses, and a host of similar ephemeral literature, might be gathered and deposited in the national library. Mr. Cable, by his novels, has done much to preserve the quaint Creole Louisianian speech; Mr. Johnston has in the same way done as much for the Georgian dialect; Miss Murfree for the Tennessee mountaineers; Mr. Page for the Virginians; a host of writers, *imprimis* Mr. Lowell, for the New-Englanders; and, not to go over the long roll of writers in American dialects, Mr. Harris has shown us what a wealth of folk-lore and folk-speech there is to be garnered among the southern negroes. But the next generation will have no such easy task as the present one. Even in slow-going England the Folk-lore society and the English dialect society came quite late enough into the field, and found that in a few years more the school boards and the desire to be 'genteel' would have effectually effaced those old-world differences of tongue which even in 1598, when Puttenham was writing his 'Arte of English poesie,' had begun to be blurred. Already many a precious relic of the past has been forever lost, and we can only be thankful that so much has been preserved. In America—I speak, of course, of the old colonial sections—there still linger peculiarities, and even bits of folk-lore, which have vanished out of the districts in the mother-countries from which the immigrants came. Now, therefore, is the time for snatching up what still remains, and I question whether there are not in the United States and in lower Canada quite as many dialects as there are in England. The 'Pennsylvania Dutchman' has even yet peculiarities in speech easily detected by those who know them, and there is scarcely an old state of the Union of which the same could not be said.

R. B.

Streatham, London, Eng., April 30.

Geography-teaching.

The article by Inspector Jolly, on 'Realistic and dramatic methods in teaching geography,' to which you refer in your number of May 12, is without doubt a clear and full statement of the various points of weakness in such work, and of the remedies to be applied.

He urges a greater use of material and a more rational and scientific method. On these two points hangs the whole matter. Every one who has ever taught geography knows that nothing can be done without an abundance of aids in the way of objects, pictures, models, globes, maps, etc.; and every one who has taught in the United States knows that objects, pictures, models, globes, and good maps are there very, very few.

A full assortment is not found in one single school; a good assortment, only in a small number, where men of wide views have had charge. There are two reasons for this condition of things,—one, that few schools take enough interest in the subject to procure what material can easily be had; the other,

which partly accounts for the first, that there is nowhere in this country any place where even an idea of what material there is, can be got. In short, we do not have good material, because we do not know what good material is.

Supposing, however, that all schools were fully equipped in that line, there arises the other issue, have we teachers who could properly use the material, and in a scientific method produce in a pupil's mind that happy result so much talked of, so seldom seen? To this there are two answers, — yes and no. The first applies to teachers who would instruct the elementary classes.

If the average normal-school graduate had been properly trained by a broad-minded instructor in the use of material, and made thoroughly acquainted with the general facts of geography and its brother-studies, botany, zoology, ethnology, etc., such graduate would be, in the primary and intermediate schools, fully competent to do the work. But in higher work, where scientific deduction should be employed, where a wide and deep knowledge on the part of the instructor is demanded, the average normal graduates could not do the work. They are not mature enough, they do not know enough. I mean what I say, when I say they do not know enough.

They are not to blame. Geography needs a fund of general information and of special information as wide as a church-door and as deep as a well. No teacher whose specialty is not geography ever acquires it, and we have almost none who are devoted to this one subject. The class-room system forbids.

This upper stage of the work needs the mature strength of college graduates, and of college graduates devoted to geography. Of such there are almost none.

In fact, I know of a vigorous attempt recently made to find one, which ended in failure. Germany alone provides her schools with such men. There one must go to know the whole subject.

These two points, then, being stated, there appears to me but one way out. The best mode of reforming the lower-grade teaching is available. Material should be brought from the centres of geographical interest abroad, and the school public made aware of the resources to be had. Then there might be an advance there.

As to teachers for the upper grade of geography, until our colleges take a higher stand in regard to requirements in the subject, and provide professors who can teach the subject so that their students will

have a real, living interest in the matter when they leave college, — until then we must wait, content with the few men who, of their own accord, work up the subject from a professional stand-point, and in their own circle of influence do really teach geography.

C. H. LEETE.

New York, May 14.

Queries.

4. TEST FOR OLEOMARGARINE. — Please give a simple test for distinguishing butter from oleomargarine. — P.

[There is no simple test for distinguishing butter from oleomargarine, — a test which at the same time is simple and accurate, and which settles the question beyond doubt. A great many tests have been proposed from time to time, but they either require special skill and apparatus for their execution, or they are of very little value, failing to accomplish what they promise. The following test will perhaps be found of some use: a cotton wick is saturated with melted fat from the butter to be tested; the wick is lighted, allowed to burn for a short time, and then blown out. If the sample is oleomargarine or adulterated butter, an offensive odor, as of an extinguished tallow candle, will be perceived. It is to be noted, however, that pure butter which has stood for a long time will give the same smell. Another test is the following, devised by J. Horsley: a little of the clear, melted fat is poured into a small test-tube; the fat is dissolved in common sulphuric ether, and about thirty drops of spirit of wine are then added; if natural butter, a white precipitate will be formed; if artificial butter, the solution will remain clear. While these tests may sometimes prove efficient, they will often leave the point unsettled. Other tests proposed for discrimination between oleomargarine and natural butter may be of more value, but, calling for special apparatus and solvents, they can hardly be called practical or simple. Chemical analysis of suspected samples will decide the question beyond dispute: outside of the chemical laboratory we have as yet no practical means of fully ascertaining whether a sample of butter is natural or artificial. — Ed.]

5. A SQUARE PUZZLE. — Having a rectangle nine by sixteen, is it possible by one cut to make two figures which joined shall make a square twelve by twelve? — Z.

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SCIENCE.—SUPPLEMENT.

FRIDAY, MAY 20, 1887.

ABSTRACT OF THE RESULTS OF THE INVESTIGATION OF THE CHARLESTON EARTHQUAKE.¹

THE amount of information now in possession of the U. S. geological survey, relating to the Charleston earthquake, is very much larger than any of similar nature ever before collected relating to any one earthquake. The number of localities reported exceeds sixteen hundred. The sources of information are as follows: 1°, we are deeply indebted to the U. S. signal service for furnishing us the reports of their observers; and, 2°, equally so to the Lighthouse board, which has obtained and forwarded to us the reports of keepers of all lighthouses from Massachusetts to Louisiana and upon the Great Lakes; 3°, to the Western union telegraph company, which instructed its division superintendents to collate and transmit many valuable reports; 4°, to the Associated press, which has given us access to the full despatches (with transcripts thereof) which were sent over the wires centring at Washington during the week following the earthquake; 5°, to geologists and weather-bureaus of several states, who have kindly exerted themselves in this matter, and collected much important information; 6°, to a considerable number of scientific gentlemen who have distributed for us our circular letters of inquiry in special districts, notably, Profs. W. M. Davis, C. G. Rockwood, J. P. Lesley, T. C. Mendenhall, and Messrs. W. R. Barnes of Kentucky and Earle Sloan of South Carolina; 7°, to a large number of postmasters in the eastern, central, and southern states; and, finally, to hundreds of miscellaneous correspondents throughout the country.

In collecting this information, a printed list of questions was prepared. This practice has been resorted to in Europe and in Japan with considerable success, and the questions which have been devised for distribution in those countries have been prepared with great skill by some of the ablest investigators of earthquakes. Prof. C. G. Rockwood of Princeton has also been in the habit of distributing formal questions of this character in this country whenever apprised by the newspapers of a notable shock. Availing ourselves of

his advice and assistance, questions prepared by him were printed and widely distributed. They were much fewer and more simple than those employed in Europe, because European investigators depend almost wholly upon the educated classes to answer them, while in this country the uneducated but intelligent and practical classes of the people must be the main reliance. These questions were designed to elicit information, 1°, as to whether the earthquake was felt; 2°, the time of its occurrence; 3°, how long it continued; 4°, whether accompanied by sounds; 5°, the number of shocks; 6°, general characteristics which would serve as a measure of its intensity, and indicate the kind and direction of motion.

It is to be observed that the only information to be hoped for, which can have even a roughly approximate accuracy, is the time of transit of the shock. The degree of approximation in the time data actually obtained will be adverted to later. Special effort was made to obtain information as to the relative intensity of the shocks in all parts of the country. At the very outset a serious difficulty presents itself. In the estimates of intensities there is no absolute measure. What is really desired is some reliable indication which shall serve as a measure of the amount of energy in any given portion of the wave of disturbance as it passes each locality. The means of reaching even a provisional judgment are very indirect, and qualified by a considerable amount of uncertainty. To estimate the force of a shock, we have no better means than by examining its effects upon buildings, upon the soil, upon all kinds of loose objects, and upon the fears, actions, and sensations of people who feel it. In view of the precise methods which modern science brings to bear upon other lines of physical research, all this seems crude and barbarous to the last degree. But we have no other resource. Even if it were possible to obtain strictly comparative results from such facts, and decide with confidence the relative measure of intensity which should be assigned to each locality, we should have gained measures only of a series of local surface intensities, and not of the real energy of the deeply seated wave which is the proximate cause of the surface phenomena. Notwithstanding the indirect bearing of the facts upon the real quantities we seek to ascertain, and their apparently confused and distantly related character, they give better results than might have been supposed. When taken in

¹ Read before the National academy of sciences at Washington, April 19, 1887.

large groups, they give some broad indications of a highly suggestive character; and though affected with great inequalities, which for the time being seem to be anomalous, these anomalies are as instructive as the main facts themselves.

We have given the preliminary plotting of the intensities in the map before you. The first point to which we shall invite attention is the magnitude of the area affected by the shocks. It was sensibly felt in Boston, which is the most distant point on the Atlantic coast from which affirmative reports have been received. From Maine the answers are all negative. Most of those from New Hampshire are negative, but two or three positive ones show clearly that it was felt in sensitive spots. In Vermont, affirmative reports come from St. Johnsbury and Burlington on Lake Champlain. No positive reports come from the Province of Quebec. In New York state it was felt in the vicinity of Lake George, and at Lake Placid and Blue Mountain Lake in the Adirondacks. In Ontario it was quite noticeable in several localities, though the great majority of reports from that place are negative. In Michigan it was noted in several places; and at Manistee lighthouse, on Lake Michigan, the trembling was strongly marked. In Wisconsin, though many of the reports are negative, it was felt quite strongly at Milwaukee, and was also noticed at Green Bay and at La Crosse on the Mississippi, 967 miles from Charleston, — the remotest point within the United States which has given a positive report. In central Iowa and central Missouri it was unmistakably felt. In Arkansas the eastern portion of the state from sixty to seventy-five miles west of the Mississippi gives numerous positive reports. In Louisiana the reports are mostly negative, but numerous persons in New Orleans felt the shocks, and recognized their nature. In Florida it was universally felt, and in the northern part of the state was severe and alarming. From the Everglade region, of course, no reports have been received, as it is uninhabited; but in some of the Florida Keys it was felt in notable force. From Cuba a few reports have come; and the most distant point in that island which was shaken was Sagua la Grande, where the vibration was very decided. Lastly, a report comes from Bermuda, a thousand miles distant from Charleston, which leaves little doubt that the tremors were sensible there.

The area within which the motion was sufficient to attract the attention of the unexpectant observer would be somewhat more than circumscribed by a circle of a thousand miles radius; and the area of markedly sensible shaking, would, including the oceanic area, be somewhere between

two and one-half and three million square miles. In this estimate, however, only well-defined seismic movement of notable force is considered. There are reasons for believing, that, by proper instrumental observation, the movement could have been detected over a much greater area. In the first place, it is to be noted that the peripheral portions of the observed area lie in districts which are rather thinly populated, sometimes also in districts which, from the nature of the ground, do not disclose forcibly the passing shock. Furthermore, the passing wave in the outer portions of the area was almost everywhere of an undulatory character and of great wave-length, and, while still retaining a large amount of energy, did not often dissipate itself into those smaller and shorter tremors which are very much more likely to attract attention, though really possessing very much less energy. Six hundred miles from the origin the long swaying motion was felt, and was often sufficient to produce seasickness, yet was unaccompanied by sound or by the tremulous motion due to short waves.

It will be observed upon the map that there are several large tracts which show a comparatively feeble intensity, while completely surrounding them is the general area of greater intensity. The most conspicuous of these areas of silence is the Appalachian region. The facts here are extremely interesting and suggestive. It has been generally supposed that a mountain-range serves as a barrier to the propagation of earthquakes, not from any known relation of cause and effect, but merely as the result of observation. In Japan it is universal testimony that the central range of the island marks the dividing-line between earthquake and no earthquake. The shocks, so frequent there, are seldom noticed beyond the mountains. A similar conclusion has been drawn from South American earthquakes, and also from those which have visited southern Italy. As soon as the data in the earlier stages of the inquiry began to indicate insulated areas of minimum action, they were completely investigated, and every effort has been made to secure full data from them. The result has been to show satisfactorily that such was the case. The Appalachian belt south of middle Pennsylvania disclosed a few spots where the shaking was considerable; but in the main it was but lightly affected until we reach the extreme southern portion of this range, where the shocks begin to be somewhat vigorous, even in the mountains. West and north-west of the range, however, the force of the undulations resumes even more than its normal vigor. In eastern Kentucky and south-eastern Ohio the force of the shocks was very considerable

causing general alarm. Chimneys and bricks were shaken down, and the oscillation of the houses was strongly felt. In south-eastern Ohio nearly every theatre, lodge, and prayer-meeting was broken up in confusion. It does not appear that the Appalachians offered any sensible barrier to the progress of the deeper waves, but it does appear that they affected in a conspicuous degree the manner in which the energy of the waves was dissipated at the surface. Another minimum area was found in southern Indiana and Illinois, and also in southern Alabama and Mississippi. There is a curious circumstance connected with the minimum area in Indiana and Illinois. On the 6th of last February an earthquake of notable force occurred in just this locality. Circulars were sent out at once, and, on plotting the isoseismals, they showed a singular coincidence in almost exactly filling the vacancy or defects of intensity of the Charleston earthquake. At present there is nothing to indicate whether this coincidence is accidental, or whether there is some hidden relation.

Where the waves passed into the newer delta region of the lower Mississippi, the surface intensity of the shocks rapidly declined. This is indicated in the map by the compression of the isoseismals in those localities. We incline to the opinion that this sudden diminution of the intensity is due to the dissipation of the energy of the waves in a very great thickness of feebly elastic, imperfectly consolidated, superficial deposits. It is a matter of common observation in all great earthquakes, that the passage of the principal shocks from rigid and firm rocks into gravels, sands, and clays, is, under certain circumstances, attended with a local increase in the amplitudes of the oscillations and in the apparent local intensity and destructiveness; and the reason for it is intelligible. But, where such looser materials are of very great thickness and great horizontal extent, the reverse should be expected: for, when a wave passes from a solid and highly elastic medium into a less solid and imperfectly elastic one, the amplitude may be suddenly increased at the instant of entering; but so rapid is the extinction, that, if the new medium be very extensive, the impulse is soon dissipated.

Many reports throughout the central states indicate localities of silence which are not expressed upon the map. The reason for omitting them is, that it has been impracticable to secure a sufficient density of observation (i.e., a sufficient number of reports per unit area) to enable us to mark out and define these smaller areas with very great precision. To do this for the whole country would require some tens of thousands of observations and

the expenditure of tens of thousands of dollars to systematize and discuss the data. A map shaded to show the varying intensity by varying the depth of the shading would have a mottled appearance, in which the mottling would be most pronounced in the areas of a little below the mean intensity, say, between the isoseismals 3 and 5. This fact is of great importance in the interpretation of the isoseismals, for the omission to consider it results in giving to the middle isoseismals too high a value. In any isoseismal zone, what we should like to ascertain is the mean intensity of the whole area included within that zone. As a matter of fact, the data we possess consist more largely of maximum than of minimum or average intensities, and therefore tend to considerably augment the mean derived intensity above the true mean. This will become apparent by an inspection of the map where the zones of 5, 6, and 7 intensity are disproportionately broad, while those of 3 and 4 are disproportionately narrow. We have not attempted to allow for this source of error, though fully aware of it, because we had no means of determining what allowance to make. We have drawn the lines wholly upon the face of the returns, and the investigators who may attempt to utilize our results must grapple with the corrections as best they may.

Throughout the states of North Carolina, South Carolina, Georgia, and north-eastern Florida, and, in general, anywhere within about two hundred and fifty miles of the centre, the energy of the shocks was very great. At Columbia, Augusta, Raleigh, Atlanta, and Savannah the consternation of all people was universal. The negroes and many of the poor whites were for a week or two, not exactly 'demoralized,' but intensely moralized, giving themselves to religious exercises of a highly emotional character; the stronger and deeper natures among them being impressed with a feeling of awe, the weaker natures with a feeling of terror. And this was general throughout the large region just specified. In all of the large towns within two hundred miles of Charleston, more or less damage was suffered by houses and other structures. Walls were cracked to such an extent as to necessitate important repairs, dams were broken, chimneys were overthrown, plastering shaken from ceilings, lamps overturned, water thrown out of tanks, cars set in motion on side-tracks, animals filled with terror, fowls shaken from their roosts, loose objects thrown from mantels, chairs and beds moved horizontally upon the floor, pictures banged against the walls, trees visibly swayed and their leaves agitated and rustled as if by a wind. These occurrences were general, and were more strongly marked, until they became

terrifying and disastrous as the centre of the disturbance was approached. At Augusta, 110 miles distant from the epicentrum, the damage to buildings was considerable, and at the arsenal in that place the commanding officer's residence was so badly cracked and shattered as to necessitate practical reconstruction. In Columbia, 100 miles distant, the shock was very injurious to buildings, and appalling to the people, but no substantial structures were actually shaken down. In Atlanta, 250 miles distant, there was no worse injury than falling chimneys and some slight cracks in the walls; but the houses were instantly abandoned in great alarm and confusion by their occupants, and many preferred passing the night in the streets to re-entering their dwellings. At Asheville, N.C., 230 miles distant, and at Raleigh, 215 miles distant, the shocks were quite as vigorous as at Atlanta.

Coming nearer the seismic centre, we find the intensity increasing on all sides. The region immediately about the epicentrum in a great earthquake always discloses phenomena strikingly different from those at a distance from it; and the differences are not merely in degree, but also in kind. The phenomena characteristic of the epicentral area cease with something like abruptness as we radiate away from the epicentrum. The central phenomena are those produced by shocks in which the principal component of the motion of the earth is vertical. Proceeding outwards, these predominating vertical motions pass, by a very rapid transition, into movements of which the horizontal component is the greater, and in which the undulatory motion becomes pronounced. The epicentrum, and the zone immediately surrounding it, is the portion of the disturbed tract which merits the closest attention; for it is here that we may find the greatest amount of information concerning the origin and nature of the earthquake. To appreciate this, we will venture to offer some theoretical considerations.

Allusion has already been made to the indefinite character of the data used for estimating the intensity of the shock. There is no unit of intensity which is at present available. In selecting certain effects of an earthquake to characterize varying degrees of intensity, the most that can be hoped for is a means for discriminating whether the relative energy of a shock is greater or less in one locality than in another. But how much greater and how much less—in conformity with what law—is a problem which remains to be solved. An earthquake impulse, however, is a form of energy transmitted as an elastic wave through the deeply seated rocks, and its propagation and varying intensity are subject to the laws

of wave-motion. There must be, therefore, some typical law governing the rate at which such a wave diminishes the intensity of its effects as it moves onward. To anticipate the objection that this typical law would apply only to a medium which is perfectly elastic, homogeneous, and isotropic, while the rocks are far from being so, we reply that we have investigated the objection, and are satisfied, that, while it has some validity, the effect of these inequalities is not great enough to seriously impair the applicability of the law, nor to vitiate greatly the results to be deduced from it. The analysis we offer is a novel one. We attach considerable importance to it, and the consequences which flow from it are somewhat remarkable.

Let us suppose an elastic wave to originate at a point *C* (fig. 1), situated at the depth *q* below the

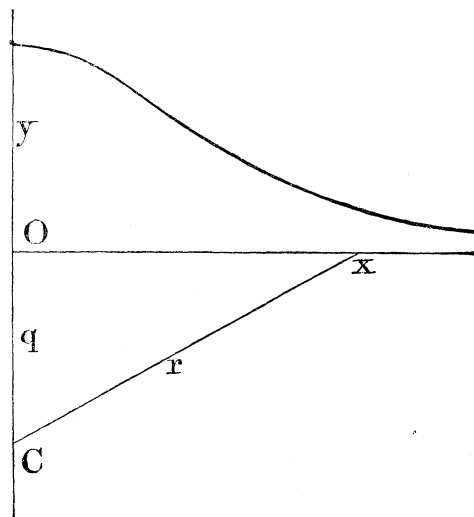


FIG. 1.

surface. Let the intensity of the shock (amount of energy per unit area of wave-front) at the distance unity from *C* be denoted by *a*. Since the intensity is inversely proportional to the square of the distance, the intensity at the epicentrum would be $\frac{a}{q^2}$. Take any other point on the surface of the earth at the distance *x* from the epicentre, and connect it with *C* by the line *Cx*. The intensity at any such point will obviously be equal to $\frac{a}{r^2}$. If we denote the intensity by *y*, we shall then have the equation,

$$y = \frac{a}{r^2} = \frac{a}{q^2 + x^2}$$

This equation expresses a curve which will serve as a graphic representation of the way in which

the surface intensity varies along a line radiating from the epicentre.

The first noteworthy feature of this curve is the contrast between the rapidity with which the intensity diminishes near the epicentre, and the slowness with which it diminishes at remote distances. Thus, at a distance from the epicentre equal to the depth of the focus, the intensity has fallen to one-half, at twice this distance it has fallen to one-fifth, and at three times the distance to one-tenth, of the intensity at the epicentre. This suggests at once the possibility of making an approximate estimate of the depth of the focus, based upon the rate at which the intensity of the shock at the surface diminishes in the neighbor-

hood of the epicentre. If we were able to construct upon any arbitrary scale whatever a series of isoseismal curves around the central parts of the earthquake with any approach to accuracy, this depth would follow at once from the relations of these isoseismals to each other. In the case of a very powerful earthquake in a region which is so flat and uniform in its features as the vicinity of Charleston, this can be done with a rough approach to accuracy.

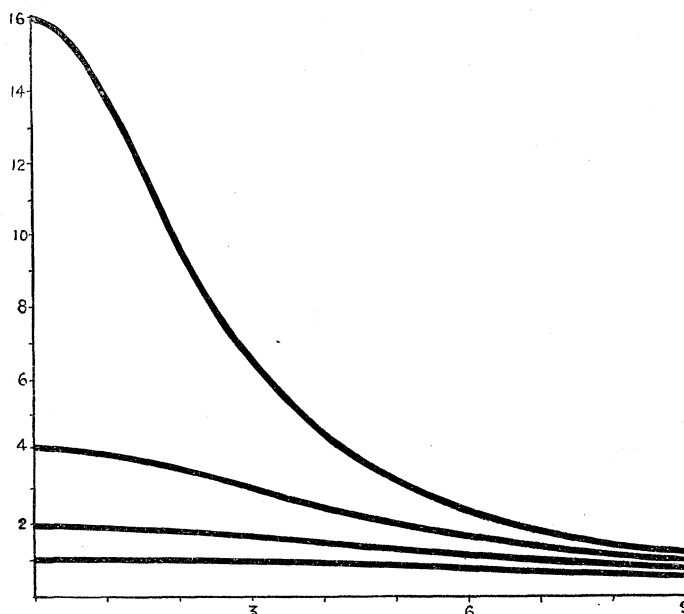


FIG. 2.—ENERGY CONSTANT, DEPTH VARYING IN RATIOS 1, 2, 3, AND 4.

hood of the epicentre. If we were able to construct upon any arbitrary scale whatever a series of isoseismal curves around the central parts of the earthquake with any approach to accuracy, this depth would follow at once from the relations of these isoseismals to each other. In the case of a very powerful earthquake in a region which is so flat and uniform in its features as the vicinity of Charleston, this can be done with a rough approach to accuracy.

To appreciate more fully the validity of this mode of reasoning, let us take a series of these intensity curves, and vary the values of the constants. And first let us suppose the total energy of the shock, measured by the constant α , remains

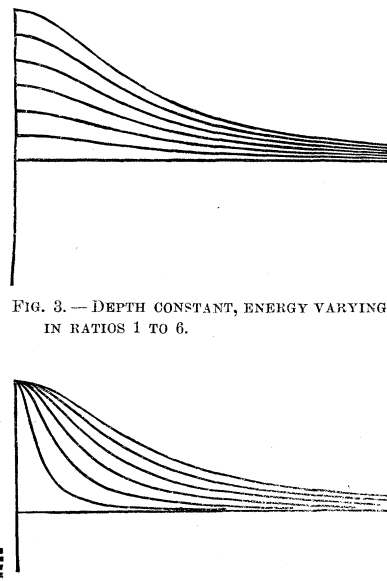


FIG. 3.—DEPTH CONSTANT, ENERGY VARYING IN RATIOS 1 TO 6.

FIG. 4.—DEPTH AND ENERGY BOTH VARIABLE, BUT WITH CONSTANT INTENSITY AT THE EPICENTRE.

method is not sensitive to small or moderate errors of observation.

The second series of curves (fig. 3) is conditioned upon the assumption that the depth remains constant, while the energy of the shock varies. In these curves, the ordinates corresponding to any abscissa are proportional to each other in a simple ratio. In the first series they are proportional to each other in a duplicate ratio.

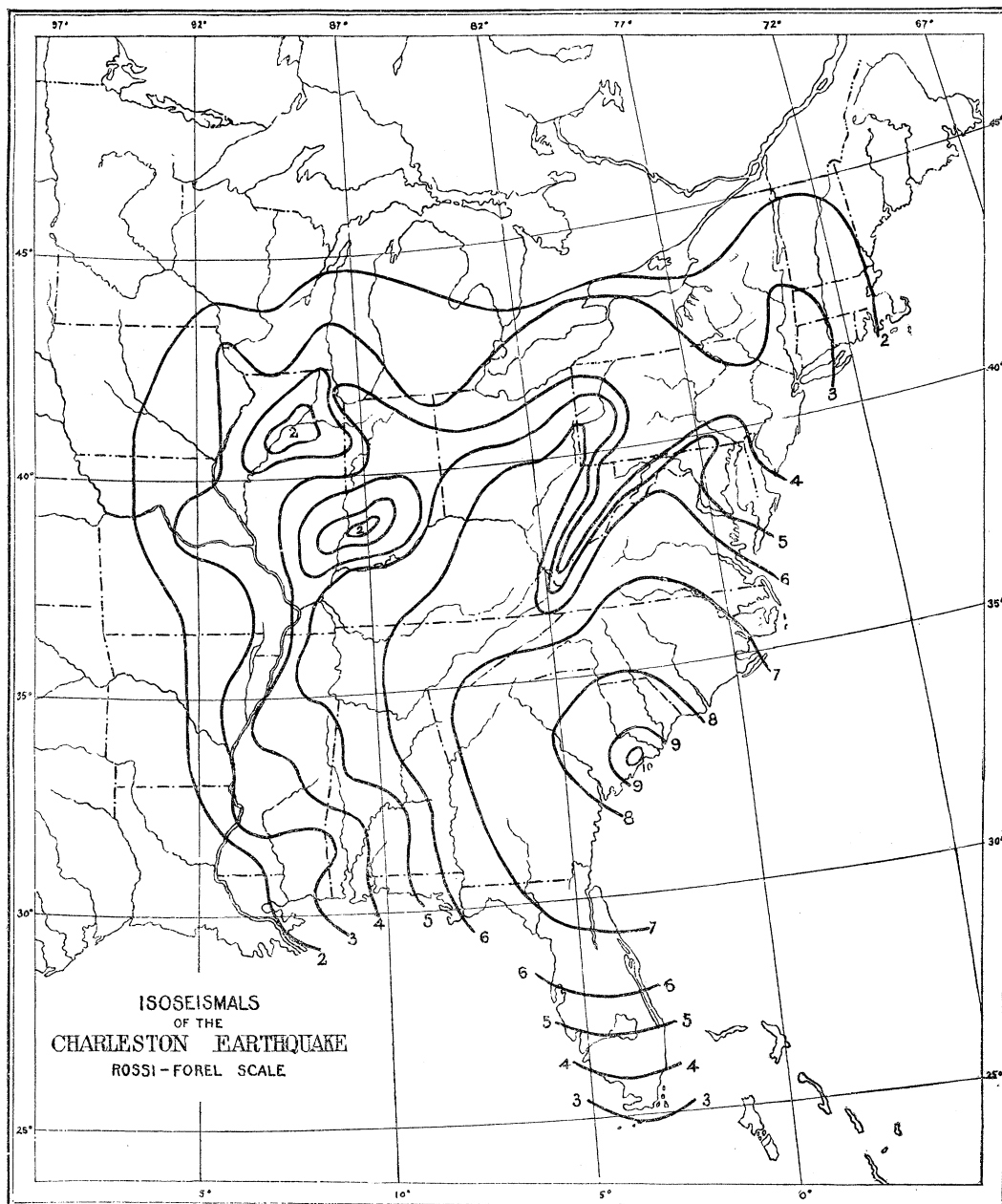
The third series (fig. 4) represents the effect of varying both the energy and the depth in such a way that the intensity at the epicentre is constant.

It will appear, therefore, that every shock must have some characteristic intensity curve, depend-

ing upon the total energy and the depth below the surface. The intensity at any point on the surface will therefore depend upon these two quan-

shock deeply seated, or to a less energetic one nearer the surface. The criterion is soon given.

It is obvious that in any shock there is some



tities, — energy and depth. It still remains to find some means of discriminating whether the intensity at any point is due to a more energetic

point at some particular distance from the epicentre at which the rate of diminution of surface intensity has a maximum value. As we leave the

epicentre and proceed outwards in any direction, the intensity diminishes, at first more and more rapidly, but farther on diminishes less and less rapidly. We wish to find the point at which the rate of decline changes from an increasing to a decreasing rate. In the curve, this point is represented at the point of inflexion, where the curve ceases to be concave towards the earth, and begins to be convex towards it. To find the co-ordinates of this point, we differentiate the equation of the curve twice, and equate the value of the second differential coefficient to zero, and deduce the corresponding value of the abscissa x ,

$$\frac{d^2y}{dx^2} = \frac{8ax^2 - 2a(q^2 + x^2)}{(q^2 + x^2)^3} = 0,$$

which equation is satisfied when

$$8ax^2 = 2a(q^2 + x^2),$$

whence

$$\pm x = \frac{q}{\sqrt{3}}.$$

In this value of x it is seen that the constant a has disappeared; and the abscissa of the point of inflexion is therefore independent of the energy of the shock, and dependent upon the depth alone. The meaning of this is, that the distance from the epicentre to the point where the rate of decline of the intensity is greatest is simply proportional to the depth of the focus, and is the same whether the energy be greater or less. This property of the intensity curves makes us independent of any absolute standard of measurement for the intensity, and all that we require is to find with reasonable approximation the points where the intensity falls off most rapidly. The depth of the focus follows at once.

The determination of the epicentral tract is chiefly the work of Mr. Earle Sloan of Charleston, a young civil engineer, who, immediately after the disaster, made an extensive series of observations. In the brief time at his disposal he accumulated a surprisingly large amount of detailed information, and in searching for it exercised a discrimination and sagacity which would have been highly creditable to the most experienced and learned observer. It is to be regretted that his business engagements prevented him from continuing the work. As it is, he has located with considerable precision the epicentral tract, and has furnished data which show well the variation of intensity along several lines radiating from it.

The summary obtained from the examination of Mr. Sloan's data is as follows: the tract which includes the most forcible action of the earthquake is an elliptical area about twenty-six miles in

length, and with a maximum width of about eighteen miles. The major axis of this area is not a straight line, but a curve, which is concave towards Charleston, and is situated from fourteen to sixteen miles west and north-west of that city. Along this line there are three points, each of which has all the characters of an epicentrum, determined by as many distinct shocks, each having a focus of its own. Much the most powerful shock centres in the northernmost focus, though the other two were of sufficient energy to have occasioned great havoc if either of them had occurred alone. The southernmost was also considerably more energetic than the middle one. The distance between the northern and southern epicentrum was about twelve miles. Within this tract, except near the edges of it, the motion was most conspicuously of subsultory character; i.e., motion in which the vertical component predominated over the horizontal. The marginal portions of this area, where the character of the movement changes, and where the intensity falls off most rapidly, seem to be very well indicated. The positions where the intensity most rapidly declines may be located with an error not exceeding one or two miles on both sides of the epicentres. The South Carolina railroad crosses the tract in a straight line very near the most forcible seismic vertical. The first point where the intensity falls off with greatest rapidity is near the nine-mile post, measuring from the railway depot in Charleston; and so well marked upon the ground are the indications of this change, that it seems very improbable that this point is more than a mile distant either way from the precise point we seek to locate. Passing north-westward through Summerville to the opposite side of the tract, we find the corresponding point of most rapid decline in the vicinity of the twenty-third-mile post. This gives us a base-line with which to measure the depth of the focus of the principal shock. The computed depth is twelve miles, with a probable error of one or two miles. The computed depths of the other foci are about the same, but the probable errors are somewhat larger.

In speaking of a focal point of a shock, it must be understood as referring to the centre of all the forces, considered with reference both to amount and direction, which constitute a great seismic impulse. The presumption is, that this impulse originates in a large subterranean tract of which this ideal focus is merely the central point, or nearly so. The form of the subterranean tract may be any thing, and, within limits, may have its three dimensions (length, breadth, and thickness) of any magnitude, and bearing any ratios to each other. The form and dimensions of it, we

cannot, of course, determine, though it may be possible to obtain some notion of its most general features if the data are sufficient.

This method of computing the depth of a seismic focus is here proposed for the first time. The method employed by Mallet, which consists in finding the angle of emergence of a wave-front from the earth by studying the configuration of cracks in buildings, is believed to be pretty nearly valueless by all seismologists. There is no definite angle of emergence, of the nature he contemplates, disclosed at the surface. Certainly in Charleston there was nothing of the kind to be found. The method employed by Seebach is sound in theory, but it requires such extreme accuracy of time-determinations that very small

no means of determining; but we do not believe that it would be so affected to any great extent in such a region as South Carolina. Being independent of any absolute measures either of the surface intensity or of the total energy of the shock, the greatest difficulty of all is at once eliminated. Our own opinion of this method is, that it is incapable alike of very great precision and of very great errors.

Probably the first thought occurring to any one examining this method will be that the determination of the two required points would be liable to very large errors. But, if he will examine the varying values of the ordinates of the curve corresponding to varying values of the abscissas and of the depth, we think he will be sat-

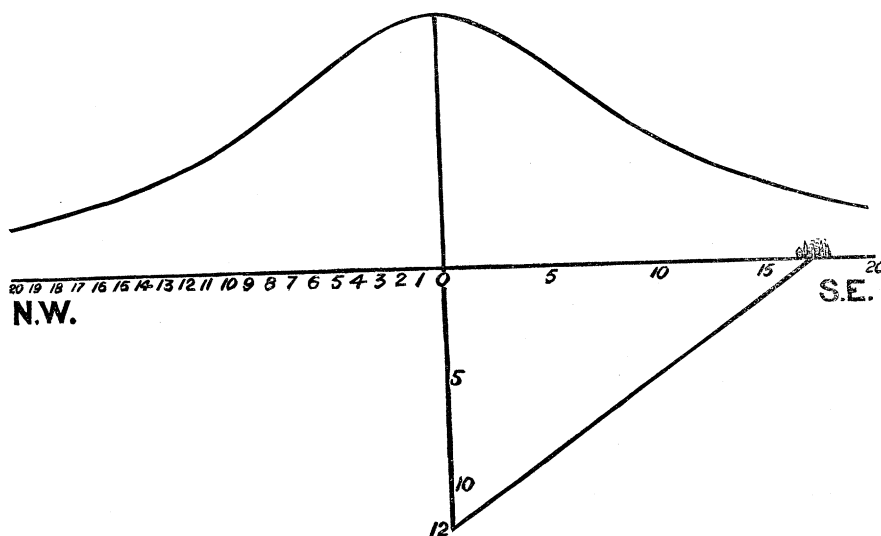


FIG. 5.—INTENSITY CURVE OF CHARLESTON EARTHQUAKE.

errors of time give very large errors in the result. Our own method consists in finding two points on opposite sides of the seismic vertical, at which the changes in seismic action along a given line are most strongly marked. These points ought to be indicated in powerful earthquakes with a fair approach to precision, and the probable errors of determination should not usually exceed one or two tenths of the distance between the two points. The feebler the shock, however, the less is the degree of precision to be expected. Whatever may be the errors in the estimate of this distance, the resulting error in the computed depth is smaller than the error of observation in the ratio of the square root of three to two. How much the estimate may be vitiated by want of homogeneity in the superficial strata, we have

assumed that the limits within which each of the two points of inflexion must fall cannot be wide apart, and that an error in the determination of the base-line greater than two-tenths of its estimated length would in such a country as Carolina be very improbable. It will appear that the relations of these variables are such as to restrict the locus within which the desired points are to be found to a very narrow annulus around the epicentrum. We believe the method will improve upon acquaintance.

We have endeavored to apply our method of computing the depth of the focus to other earthquakes, but have found difficulty in obtaining any thing more than very general results, such as the following. The depth of the Charleston earthquake was relatively great; and we find reason

for believing, that among those great earthquakes of the last hundred and fifty years, of whose effects we possess any considerable knowledge, none have originated from a much greater depth, and few from a depth so great. Our reasoning is this: very few earthquakes have been felt at a distance from the origin so great as a thousand miles; but the greatest distance at which the tremors are felt is the best measure of the total energy of the shock. On the other hand, the intensity of the Charleston earthquake in the epicentral tract was relatively low in comparison with other great earthquakes. If, then, any shock is more intense at the epicentre without extending to a greater distance than that of the Charleston earthquake, it is certain that its focus was nearer the surface. This is true of the vast majority of recent earthquakes which have been sufficiently investigated. It is suggested that all estimates of the depth of earthquake foci much exceeding twelve miles are in need of re-examination.

The city of Charleston is situated from eight to ten miles outside of the area of maximum intensity, and did not experience its most destructive power. Following the law which we have laid down, the intensity of the shock at Charleston was only three-tenths what it must have been at the epicentrum, and about one-third the intensity at Summerville. The diagram (fig. 5) showing the long intensity curve stretching from Charleston to a point forty miles north-west of it, will illustrate the position of the city with reference to the varying force of the shock.

Had the seismic centre been ten miles nearer to Charleston, the calamity would have been incomparably greater than it was, and the loss of life would probably have been appalling. Another circumstance greatly broke the force of the shocks. All of the coastal region of the Carolinas consists of a series of clays and quicksands, which have been penetrated by artesian borings to a depth of two thousand feet, and which are believed to have a much greater thickness. These beds of loose material, no doubt, absorbed and extinguished a considerable portion of the energy of the shocks. We have already remarked that a wave passing from firmer and more elastic material into material less firm and elastic, produces at first an increased amplitude of wave-motion which is liable to be more destructive or injurious to buildings. But, if the mass of less consistent strata be very great, the reverse result is produced, by reason of the rapid extinction of the energy in passing through a considerable length or thickness of very imperfectly elastic material. We cannot but think that Charleston owes in some measure its escape

from a still greater calamity to the quicksands beneath the city.

Another aspect of the same fact, if such it be, is found a hundred miles west and north-west of Charleston. Here the loosely aggregated sediments of tertiary and cretaceous age which cover the Carolina coastal plain have thinned out, and the crystalline rocks appear at the surface, thinly covered with soil and alluvium. All along the junction of these loose strata and superficial material with the metamorphics, the intensity of the shocks was conspicuously greater than to the eastward and southward. The loose covering of these firm rocks is just thick enough to give full effect to the increased amplitude of vibration which occurs when the wave passes from very solid and elastic rocks to those which are less so.

We have also endeavored to reach some trustworthy estimate of the amplitude of movement at the surface, but the results are meagre and far from satisfactory. The 'amplitude of the earth particle' in any earthquake is a question of great practical importance, and it is much to be regretted that no better facilities for determining it can be obtained. There were, however, many occurrences at Charleston bearing upon this question, which are extremely difficult to explain upon any valuation of the amplitude less than ten inches to a foot. Such amplitudes, however, were most probably limited to spots here and there, while in other spots it was probably much less. That within a small area the amplitude of movement in the surface soil varies between very wide limits, seems to be a practically certain conclusion from the observations. In Charleston it appears to have been greatest in the 'made ground,' where ravines and sloughs were filled up in the early years of the city's history. The structures on higher ground, though severely shaken, did not suffer so much injury.

With regard to the time data from which the speed of propagation must be computed, we are not yet in a position to give final results, but can only state how the problem stands at present. The time reports have been placed in the hands of Professors Rockwood and Newcomb, with the request that they would scrutinize and discuss them. But neither has been able to finish, as yet, the task he has so courteously undertaken. Probably the greatest difficulty in the way of determining the speed of propagation arises from the ill-defined character of the disturbance at considerable distances from the origin, and from the very considerable duration of it. Wherever a time observation seems to be well authenticated, there still remains, in most cases, the difficulty of deciding to what particular phase of the earthquake the

record refers; and this difficulty is a very serious one. At Summerville the first shock came almost like an explosion. Before people had time to think, they were pitched about like ten-pins. At Charleston there was a perceptible interval, estimated at from five to eight seconds, from the first note of warning to the maximum of the great shock. At Savannah (90 miles distant) the interval from the beginning to the first maximum was considerably longer, probably ten to twelve seconds; at Augusta (115 miles) the interval was still greater; and, generally speaking, the greater the distance, the more the phenomena were 'long drawn out.' The duration of the earthquake at Charleston will probably never be known with accuracy, but the general testimony ranges between fifty and ninety seconds. At Washington (450 miles) Professor Newcomb, with his watch in his hand, observed a duration of perceptible tremors with two maxima lasting about five and one-half minutes. Professor Carpmal's magnetographs recorded the disturbance, and he interprets their photographic traces as showing a duration of about four minutes. Mr. G. W. Holstein of Belvidere, N.J., gives five minutes very nearly as the observed duration. From other localities come well-attested observations showing durations of several minutes, though few of these pretend to give the whole time with any accuracy. This progressive lengthening of the shocks is a well-marked feature of the testimony. The explanation suggests itself at once. The elastic modulus of compression being greater than that of distortion, the speed of the normal waves is the greater, while the waves of distortion lag behind.

It is obvious that the phase which it is desired to observe should be the arrival of the first impulses, but the great duration of the tremors has left much doubt on this point. Stopped clocks were plentiful all over the country, but at what phase of the earthquake did they stop? So great, indeed, are the uncertainties on this point, that the observations of intelligent men, with watches in their hands, measuring a part of the shock and estimating the beginning, are in most cases to be preferred to stopped clocks, even if we knew with certainty that the clocks had been accurate to the second. It matters little how we twist and turn the time data: the smallest estimate we can put upon the speed of propagation must prove to be a great surprise to seismologists.

The time at Charleston of the occurrence of the main shock has been fixed at 9.51.10 P.M., 75th meridian, or eastern standard time (all times in this paper, unless otherwise specified, are reduced to that meridian). The uncertainty does not exceed ten seconds. The beginning of the

first tremors at Charleston was from six to eight seconds earlier. The time at Summerville was probably less than four seconds earlier than Charleston. For all localities within two hundred miles, the time observations are of little value. So swiftly did the waves travel, that a small error in the time record gives a very large uncertainty in the resulting speed.

The nearest point which yields a valuable record is Wytheville, Va. (286 miles).¹ Mr. Howard Shriver was sitting at a transit instrument, waiting for the passage of a star, and at once noted the time at 9.52.37 (reduced to 75th meridian), giving a speed of about 3.3 miles (5,300 metres) per second. There is some slight uncertainty about the precise phase of the shock corresponding to the observation.

The signal service observer at Chattanooga (332 miles) gives only the nearest minute for the principal shock at 9.53, corresponding to a speed of 3.02 miles per second, or 4,860 metres.

The best observation in our possession is that of Prof. Simon Newcomb himself, at Washington (450 miles), who gives the time of the beginning of the shock at 9.53.20, with an uncertainty not greatly exceeding ten seconds. The resulting speed is 3.46 miles per second, or 5,570 metres.

From Baltimore (486 miles) Mr. Richard Randolph, C.E., reports a very intelligent and carefully verified observation of 9.53.20 as the beginning of the shock, — exactly Professor Newcomb's time for Washington, giving a speed of 3.74 miles, or 6,000 metres, per second.

At Atlantic City, N.J. (552 miles), a large pendulum-clock in the Fothergill House stopped at 9.54 very nearly. If this may be taken to be the beginning of the shock, the speed would be 3.26 miles per second, or 5,250 metres.

George Wolf Holstein, Belvidere, N.J. (622 miles), gives 9.54 for the beginning of the shock, and 9.59 for the end, and compared his watch next morning with the time of the Pennsylvania railroad. The gradual and uncertain character of the beginning and end would not admit of precise determination to seconds. The speed, taking 9.54 for the beginning, would be 3.66 miles, or 5,900 metres.

From New York City (645 miles) and its suburban towns and cities come many reports, all of which give either 9.54 or 9.55 as the nearest minutes. If we take as a mean 9.54.25 at New York and Brooklyn for the beginning of the shock, the speed would be 3.31 miles, or 5,330 metres.

¹ The distances are measured somewhat hastily with a scale upon the war department map of the United States, taking the greater epicentrum 16½ miles north-west of Charleston as the starting-point.

At distances greater than six hundred miles, the difficulty of associating the time records with particular phases of the shocks becomes very great. In most cases the motion was the swaying movement, with only faint tremors of the rapid kind; and those who felt them were slow in recognizing their character. Readers must form their own opinions as to the degree of approximation to the time of the earliest movements, from the following records. We give them only as we received them, without attempting any discussion.

J. O. Jacot, watchmaker and jeweller at Stockbridge, Mass. (772 miles), was sitting by his regulator-clock, distinctly recognized the nature of the movement, and noted the time as 9.56. The phase of the shock is uncertain.

At Albany, N.Y. (772 miles), Mr. J. M. Clarke, of the New York state museum of natural history, heard the mortar falling down the chimney and the creaking and straining of the building. As soon as he appreciated the character of the disturbance, he noted the time by his watch at 9.56.30. He did not ascertain the error of his watch. In the same city, Dr. Willis G. Tucker says he instantly looked at his watch, and after comparing it next morning with the time of the Dudley observatory, and making correction of the error, gave 9.55 very nearly, with an error probably not exceeding twenty seconds.

From Fonda, N.Y. (780 miles), Francis L. Yates reports 9.55 (no particulars).

At Ithaca, N.Y. (695 miles), the regulator clock on the wall of the railway-depot stopped at 9.55 'exactly.'

At Gowanda, N.Y. (666 miles), where the shocks were faintly felt, W. R. Smallwood, watchmaker and jeweller, noted the end of the perceptible shocks at 9.55.30 by his regulator-clock.

At Toronto (753 miles) the earthquake was recorded automatically upon the magnetographic traces in the observatory of Prof. Charles Carpmael, superintendent of the Meteorological service of Canada. In his letter of Sept. 14 he says, "I may state that at 9.55 P.M. all our magnetic needles were set in motion by earth-tremors. The vibrations of the magnets continued for about four minutes. I would say, that, from later and more careful measurements from our magnetic curves, I make the time of the earth-tremor at Toronto to be 9.54.50 P.M. standard: this time, I should say, would not be astray more than a few seconds." As this record was automatic, and gave not only the time but the phases, it has been thoroughly investigated by Professors Newcomb and Carpmael, assisted by Mr. C. A. Schott of the U. S. coast survey. The final result of this re-

examination is to change Professor Carpmael's computation to 9.56.18 for the beginning of the tremors, with a probable error of fully one minute. This large probable error is due to the very small scale upon which the magnetograph records time intervals (one-tenth of a millimetre corresponding to twenty seconds), and to want of sharpness in the photographed traces. This time gives 2.66 miles per second, or 4,250 metres, with a probable error of one or two tenths the amount.

The clock in the Western union telegraph office at Pittsburgh (523 miles) was stopped at 9.54.

From Cincinnati and suburban towns (500 miles) come many reports. In this city, local mean time is largely used, owing to the fact that it is nearly midway between the 75th and 90th meridians, where the only inconvenience of standard time is at a maximum. The correction to the 75th meridian is 37m. 40s. The Western union telegraph office gives 9.54. The *Times-star* newspaper gives from the clock in its own office 9.16 'exactly' (9.53.40 standard); at the *Commercial gazette* office, 9.17.45 local, 9.55.25 standard (probably noted after the shocks were over). At the fire-tower after the principal shock, 9.16.17 was noted; clock error twenty-three seconds slow, giving 9.54.20 standard. Two other observers noting by watches give 9.16, and one notes an advanced stage of the shocks at 9.17, but they give no means of estimating their errors. At Covington, Ky., across the Ohio River, I. J. Evans, watchmaker and jeweller, reports his regulator-clock stopped at 9.17.20, Cincinnati local mean time; phase of shock unknown.

From Crawfordsville, Ind. (622 miles), E. C. Simpson, C.E., reports through Prof. J. M. Coulter of Wabash college, "Suddenly felt my chair move, jumped up, and said, 'We are having an earthquake.' At once pulling out my watch, I found it was 8.54 P.M., standard time (central). Professor Coulter adds, that the watch was exactly with railroad time as shown at the railroad-station and also by the town-clock.

From Dyersburg, Tenn. (569 miles), Louis Hughes writes, "My time-piece was an English patent lever watch of Charles Taylor & Son, London, which from business necessity I keep closely with railroad time at the station, which receives the time at ten o'clock every morning. The railroad uses central time. My first thought was that the shaking was caused by the children in the next room, but in the next moment, recognizing the peculiar sensation, I dropped the newspaper and observed the time, which was probably from four to six seconds after 8.54, and from that approximated it in even minutes." Speed 3.35 miles, or 5,230 metres.

At Memphis, Tenn. (590 miles), the signal-service observer reports a considerable number of stopped clocks, one at 9.54, and the others at 9.55. For some unaccountable reason the seconds were not noted. The phase is unknown.

The foregoing comprise those time reports which seem to justify the presumption that the errors do not exceed one minute. There are others which are obviously rude approximations, giving exact hours, quarter-hours, or tens of minutes. There are also some which look at first like good observations, but which surely involve some large unexplained error.

As the discussion of the time data is now progressing, no further comment will be offered here, beyond the remark that there can be no doubt that the speed of propagation exceeded 3 miles, or 5,000 metres, per second. The only questions are, how much this speed was exceeded, and whether the speed along any given line was constant. As regards the latter question, the data are not yet precise enough to justify an opinion. This matter will be inquired into.

The high rate of propagation will probably prove unexpected to European seismologists. We propose, however, to follow it up with the suggestion that it is about the normal speed with which such waves ought to be expected to travel, and that all determinations of the rate of propagation in any former great earthquakes, which are much less than 5,000 metres per second, for normal waves at least, are probably erroneous in proportion as they fall short of the Charleston earthquake. Finding, as the time reports accumulated, that a speed in excess of 5,000 metres was indicated, and this presumption having become a conviction, we were led to inquire whether there were not some speed deducible from the theory of wave-motion in an elastic solid to which all great earthquakes ought to approximate.

In a homogeneous and perfectly elastic solid, the rate of propagation is, according to theory, dependent upon two properties of the medium, — elasticity and density. There are two coefficients of elasticity in solid bodies, one of which measures their resistance to changes of volume, the other to changes of form. Absolute experimental determinations of the values of these coefficients have never been made. If, however, we knew the ratios of these coefficients in one substance to the homologous coefficients in any other substance, and if we also knew the rate of propagation in either of them, the rate in the other would be at once deducible. The rate in steel bars has been the subject of much experimentation, and is given by Wertheim, whose researches have been as careful as any, at 16,800 feet per second. But,

as the waves in a steel bar are essentially waves of distortion, he multiplies this result by $\sqrt{\frac{3}{2}}$ or $\frac{5}{4}$ for the normal wave, giving a speed of 21,000 feet per second. The elastic modulus of steel for engineering purposes is usually taken to be 29,000,000. The corresponding modulus for such rocks as granite and basalt in a very compact state is about 8,000,000. If we may assume that these moduli are proportional to the two elasticities of the two substances respectively, we can compute the rate of propagation in rock. This assumption may or may not be true; but we assume it to be so. Let V_s be the rate of propagation in steel, and V_r the rate of propagation in rock, and let e_s and e_r be their true elasticities of volume, and let D_s and D_r be their respective densities. Our assumption is, that $29 : 8 :: e_s : e_r$, from which we may form the equation,

$$\frac{V_s}{V_r} = \sqrt{\frac{e_s}{D_s} \times \frac{D_r}{e_r}}$$

Taking the density of steel at 7.84, and of deeply buried rocks in their most compact state at 2.85,

$$\frac{V_s}{V_r} = \sqrt{\frac{29}{7.84} \times \frac{2.85}{8}} = 1.15 \text{ nearly.}$$

Taking the rate of compressional waves in steel to be 6,400 metres per second, gives 5,570 metres for similar waves in very compact and dense rock. The corresponding rate for waves of distortion would be 4,450 metres. These results are so near to those deduced for the Charleston earthquake that they seem to be worthy of consideration.

The experimental measurements of the rate of impulses obtained by Milne and Fouqué seem to us inapplicable. The elasticity of the surface soil, we think, is no more to be compared with that of the profound rocks which transmit the great waves of an earthquake, than the elasticity of a heap of iron filings is to be compared with that of an indefinitely extended mass of solid steel. The difference is *toto coelo*. But the rate of propagation is a question of elasticity and density chiefly. The effect of temperature we have not considered. Perhaps the most striking experiment ever made with an artificial earthquake was at the Flood Rock explosion in Hell Gate, near New York, where General Abbott found a speed of propagation approaching very closely to that of the Charleston earthquake.

The question which is undoubtedly of deepest interest in this connection is whether the Charleston earthquake throws any new light upon the origin of such events. While we are not prepared to say that absolutely nothing will be added to our information on this question, we are forced

to admit that we expect very little new light. Hitherto our efforts have been devoted to bringing together the facts, and to arranging and comparing them, and we have as yet given but little consideration to this final question. It will, however, shortly engage our attention; and, in anticipation of this, we prefer to remain silent for the present, fearing that if we commit ourselves here to any preference for a particular view, we may find ourselves encumbered with a bias arising from the intensely human propensity to defend, through thick and thin, utterances which have once been formally given.

C. E. DUTTON.
EVERETT HAYDEN.

WAGNER'S ANNUAL REPORT ON THE PROGRESS OF GEOGRAPHY.

It is always with some impatience that we expect the publication of Wagner's report on the progress of geography (*Geographisches Jahrbuch*), because we know that we shall find there a full report of the work done in the field and in the study, and that we shall have a never-failing book of reference. We do not know of any similar publication, — except the fragmentary notes published by the Smithsonian institution and in the journals of many societies, — and therefore it is indispensable to the geographer. Though *Petermann's Mittheilungen*, the leading German geographical journal, contains regular reports on recent publications, their character is different from those in the *Jahrbuch*, the reports in the journal giving a more detailed review of the single publications, and being more disconnected. The list of reviewed books is consequently not so full as that of the annual report. The latter gives a comprehensive account of the work done during the last two years. The present volume is the eleventh of the series. The editor, Prof. H. Wagner of Göttingen, has preferred to divide the material, and to publish alternating volumes, one containing the various branches of geography, the other the progress of explorations, methods and teaching of geography, etc. Through this division, the book has increased in volume and the report has become more exhaustive. The present volume contains the special part, geophysics, geognosy, oceanography, climatology, geography of plants and animals, and ethnology. The place of the late Professor Zöppritz is taken by Dr. Hergesell and Dr. Rudolph; the former report on deep-sea explorations has been enlarged so as to cover all problems of oceanography, and is given

Geographisches Jahrbuch. Vol. xi. 1887. Ed. by HERMANN WAGNER. Gotha, Justus Perthes, 1887.

by Professor Krümmel; F. Toula reports on geognosy; the other parts are in the hands of the same specialists who gave the valuable reports of former years.

In looking at the long series of reports, we find that each number served more satisfactorily the purpose of being a reliable book of reference to all interested in geography. At the present time there are few branches of geographical study which are not embraced in the book. The steady development of the plan, by dropping unnecessary parts, including in one part what belongs together, and adding new departments which had developed into important branches of science, encourages us to hope that within a few years the whole domain of geography will be represented in it. We should wish, for instance, to have an additional report on the history of geography. That on terrestrial magnetism is promised for next year. Among the important additions in the volume of 1887 is the first report of the geography of ancient Greece and the neighboring countries. The ancient geography of other countries, except that of the birthplace of our culture, is so little studied, that the contents of a biennial report would be very meagre. We hope, with the development of these studies, which are principally carried on by ethnographers, we shall find an account of these also. In 1882 Egli's reports on the study of geographical names, and S. Günther's on the theory of map-projections, were added to the book. We consider it a waste of time and work, that the Physical society of Berlin continues its reports on physical geography in the way they were given before the *Jahrbuch* had attained its present importance. It is true that they contain some material not included in the *Jahrbuch*, for instance, measurements of heights, etc.; however, these would far better find their place in the latter publication than in the reports on the progress of physics.

The rapid development of the *Jahrbuch* and the fact that every department is intrusted to the care of a specialist, make it an extremely reliable and useful book, which is a valuable help to the student of geography.

THE MECHANICS OF MACHINERY.

PROFESSOR KENNEDY is well known as one of the ablest among British workers in this field, and this volume contains a series of lectures delivered by him to his classes during the period of his connection with the University college, on a subject with which he is especially familiar. As was to be expected, the work is one of exceptional value.

The mechanics of machinery. By ALEX. B. W. KENNEDY. London, Macmillan, 16s.

The subject is the mechanics of constrained motion, and is purely kinematics.

The book is a volume of about 650 pages, and is divided into a dozen chapters. The first six chapters consider purely geometric problems in the elements of kinematics and mechanism, introducing some interesting methods of solution involving 'virtual' rotations; securing a means of treating all mechanisms, whether of rotational or of rectilinear movement, by the same system; and greatly simplifying the work. In the seventh chapter, accelerations and retardations are considered; and in the succeeding chapter, static equilibrium and work-diagrams. Then follow chapters on problems in machine dynamics, and on parallel and other familiar mechanisms, and various trains. The last chapter considers the modifications introduced by the action of friction. In these applications we find the motions of the steam-engine and its accessories, of the fly-wheel, connecting-rod, and governor, and the various sorts of gearing. The author is one of the few writers who have yet had the courage to drop the fallacious and misleading so-called laws of friction, as enunciated by earlier writers, and to introduce the results, even though very briefly, of recent research, with correct statements of the enormously differing, lately discovered laws of friction of lubricated surfaces.

Professor Kennedy follows Reuleaux, in the earlier part of his lectures, as far as opportunity and necessity dictate, but soon gets out into a field all his own, and develops his treatment in his own logical and fruitful manner.

The book is well illustrated, pictorially and by examples; the references are conscientiously introduced throughout; and the volume, as a whole, is remarkably well adapted for use as a text-book in technical schools, and will also be found very useful to the practitioner. R. H. THURSTON.

A MODEL FOR AMATEUR ASTRONOMERS.

BARON VON ENGELHARDT has recently published, in a handsomely printed and bound volume of two hundred and twenty quarto pages, a series of astronomical observations made at his private observatory in Dresden from 1879 to 1886. The observations were all made by Baron von Engelhardt himself, and they give evidence of a good observer, while the reductions have been made in a most thorough manner. It is rarely, indeed, that we find work of this character systematically carried on for so many years by an amateur; and it implies, moreover, a good deal of careful pre-

liminary training. The field chosen is not the 'new astronomy,' with its many fascinations, but the more prosaic 'old astronomy,' the astronomy of the elder Struve and of Bessel, — painstaking measurements of double stars, comets, asteroids, nebulae, and clusters, observations of moon-culminations, occultations, etc., all valuable contributions to our knowledge of the positions and motions of the heavenly bodies. Here is an excellent example for the amateur astronomers of this country. There are plenty of fine instruments in the hands of amateurs, and only a moderate amount of industry is called for, yet hardly one of these instruments is doing any thing for the advancement of science. In England there are several private observatories of world-wide reputation, in which the owner either carries on regular observations himself, or employs a competent assistant; while here, since the death of Dr. Henry Draper, the field is almost deserted.

Baron von Engelhardt built a small observatory in 1877, in which was mounted an 8-inch Grubb equatorial; but, finding this at an inconvenient distance from his home, he put up a more elaborate building connected directly with his villa on the outskirts of Dresden.

The new observatory is a three-story tower, the upper story being surmounted by a cylindrical 'dome' containing a 12-inch Grubb equatorial. The second floor connects with the transit-room, in which is a 'broken-back' transit by Bamberg of 2.7 inches aperture. The observatory is also thoroughly equipped with subsidiary apparatus, clocks, chronometers, chronograph, etc. Upon the roof of the villa is a little 'comet observatory,' where were formerly two telescopes, one of 6.4 inches aperture, and the other of 3.7 inches. The larger instrument, which is patterned after the Strassburg comet-seeker, is of somewhat novel construction: the telescope is fastened by two long arms to the back of a comfortable chair, so that the eye-end of the telescope is just at the height of the observer's eye; the arms are pivoted to the chair-back, permitting a motion in altitude, while the chair turns about a vertical axis, like an ordinary office-chair, so that the astronomer can examine the whole sky rapidly and without fatigue. The mounting for this instrument is now at the University of Kiel.

The volume before us contains a full description of the instruments, illustrated by several plates. The observations and reductions are given in some detail, and the whole work would reflect credit upon any observatory.

WORK will begin in June next on the Holstein canal, to connect the Baltic with the North Sea.

Observations astronomiques. Par B. D'ENGELHARDT. Première partie. Dresde, 1886. 4°.